A Negative Optical Proof of the Absence of Seas in Mars.
By H. Dennis Taylor.

When occupied in reading a highly interesting article on the physical configuration of *Mars* in *Astronomy and Astrophysics* an idea suddenly struck me which seemed to me a valuable one —viz. that if there exist seas, or even water-covered spaces of moderate size, within the tropical regions of *Mars*, then we should expect to see a minute but brilliant reflection of the Sun in such water spaces whenever, by the diurnal rotation of the planet, they were brought into the proper angle to send us a solar reflection. I at once entered into a few simple calculations which satisfied me that the apparent brightness of this solar reflection, analogous to a reflection of the Sun from a thermometer bulb, would at least compare on fairly equal terms with the brightness of a 1st magnitude star. I then drafted a paper on the subject and submitted it to the March Council meeting of this Society, along with a note in which I said I could scarcely believe that its matter was entirely novel, and that the idea had not occurred to anybody before. At the same time I was not aware of any books on astronomy in which the important bearing of this question upon the physical state of *Mars* was even alluded to. Since then, however, one of the referees to whom my paper was submitted has kindly referred me to the memoir of Schiaparelli on *Mars* and to a paper dealing with this very question by Professor Phillips, of York, which, it seems, was published first in the *Proceedings of the Royal Society*, vol. xii. 1863. So that the claim for priority rests, I believe, with Professor Phillips. But until the other day I was quite ignorant of this, and the fact that the very same idea should afterwards recur to me, living in the same town, seems to suggest that there is something in ideas akin to certain epidemics. In the course of discussing the nature of the markings upon *Mars*, Professor Phillips wrote : " A positive proof of ocean on the disk of *Mars* would be afforded by the star-like image of the Sun reflected from the quiet surface or the more diffused light thrown back from the waves ; but nothing of this sort has been placed on record." In a note he also wrote : "The quiet image here alluded to would not exceed $\frac{1}{20}$ of a second of angle at the opposition, if no allowance be made for irradiation." He then concludes by assuming it doubtful whether this star-like image would be visible except perhaps in very large instruments. But he enters into no calculation as to the apparent *brightness* of this "artificial star" as viewed from the Earth and as compared with other real stars.

Then Professor Schiaparelli, in his beautiful and elaborate memoir on *Mars* during the opposition of 1877, took up the subject again as being of the very highest importance. While expressing himself as very doubtful as to whether the darker

markings of *Mars* are to be regarded as seas, he then passes to the consideration of what he seems to consider the most powerful argument against their existence. I will here give a condensed translation of his method of dealing with the question. His figures are in practical accordance with what I have myself arrived at as most probable.

"It is certain," he says, "that if *Mars* were a polished sphere, capable of reflecting, in the fashion of a speculum, the solar rays, we ought to see a very minute image of the Sun glittering on his disc in the form of a star, and at a position easy to assign at each moment. Assuming the reflection to be total, it is easy to calculate the intensity of this image, given the diameter of *Mars* and its distance from the Earth and Sun. For example, in the opposition of 1877 attained on September 5, *Mars* being at the distance of 1·383 from the Sun and about 0·377 from the Earth, the image in question ought to figure as a star of a diameter of $\frac{1}{24}$ of a second of arc, and therefore of a brilliancy equal to $\frac{1}{2{,}100{,}000{,}000}$ of that of the Sun at distance 1. Now Professor Zöllner has estimated the total light reflected to us from *Mars* in mean opposition to be $\frac{1}{6{,}994{,}000{,}000}$ of that of the Sun at unit distance. From this it follows that in the opposition of 1877 the total luminosity of *Mars* was $\frac{1}{2{,}990{,}000{,}000}$ of that of the Sun at distance 1. Therefore, assuming total reflection, the minute solar image reflected from the polished surface of *Mars* would surpass in brilliancy the whole of the light from his disc. However, great allowance must be made for the low reflective power of a liquid like water, whose index of refraction being only $\frac{4}{3}$ would imply a reflective power for approximately perpendicular incidence of about $\frac{1}{49}$ part. Then allowance must be made for the absorption due to the double passage of the solar rays through the Martian atmosphere. Assuming the absorption to be as great as in the case of our own atmosphere, the above fraction ought to be reduced to about one-half, or in round numbers $\frac{1}{100}$. On this supposition, then, we should have for the luminosity of the reflected image

$$\frac{1}{21 \times 10^{10}}$$

of that of the Sun. But Zöllner has estimated the brightness of *Capella* to be

$$\frac{1}{5{\cdot}57 \times 10^{10}}$$

of that of the Sun. So that on 1877 September 5 the image of the Sun reflected from the convex mirror formed by any seas in *Mars*

ought to have appeared of a brightness equal to about one-fourth of that of *Capella, and therefore as a star of the 3rd magnitude.*"

The last words in my own italics are remarkable as seeming to show that Professor Schiaparelli regarded *Capella* as of the 1st magnitude, whereas measurements have made it of zero magnitude, or two and a half times as bright as an average 1st magnitude star. Therefore Professor Schiaparelli's magnitude 3 ought to read as magnitude 1·5. He then goes on to say that "Such a star could not possibly escape observation if occurring upon any of the darker areas of *Mars*, in spite of the undoubted influence which the surrounding bright surface of the planet would have to drown its splendour. But very slight inequalities in the surface of the water, in the shape of waves, would be enough to break up the clear united reflection into an infinitude of minute reflections. Now it is true that the sum of the luminous values of these reflections would not turn out much different to the single image, but the light would be spread over a larger space, and the amount of extension would depend upon the form of the waves and the inclination of their sides to the horizon. The formation of rugged crests would destroy any real image, and the nebulous patch of light having but a fraction of the luminous value of the light from the disc, might easily escape observation." Seemingly, then, Professor Schiaparelli concludes that the solar reflection could only fail to be visible to a spectator on the Earth armed with a good telescope, by reason of the water areas of *Mars* being in a rough and turbulent state.

But it would have to be assumed that the Martian water surfaces are *always* rough, and have always been rough ever since the invention of the telescope, which supposition is, I venture to think, quite untenable, if not ridiculous, in face of all we know about our own oceans, lakes, and rivers, and also certain facts, more clearly established of late, with regard to the behaviour of the so-called seas of *Mars* and the density of the Martian atmosphere.

Before returning to the discussion of the question, of the utmost degree of roughness allowable in the water areas of *Mars* consistently with the visibility of the solar reflection, I propose to slightly supplement Professor Schiaparelli's calculations of the brightness of the solar reflection by a different method.

Such various determinations have been arrived at by different observers as to the stellar magnitude of the Sun, or the ratio between its apparent luminosity and that of a star of zero magnitude like *Vega* or *Capella*, as to raise some doubts as to the degree of accuracy to be assigned to the above calculations, although the photometric estimates of the Sun's brightness arrived at by Zöllner certainly seem to accord fairly well with the apparent brilliancy of the planets.

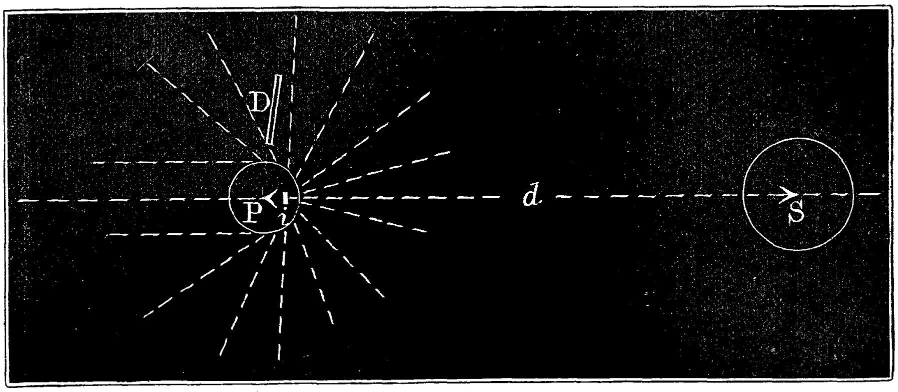

Fig. 1.

In fig. 1 let S be the Sun of a radius R, P a polished sphere of radius r illuminated by S at the distance d separating centres of S and P. And let D represent a disc, also of a radius r equal to that of P, which is placed at the same distance d from S, and illuminated perpendicularly by the latter. Let the surface of D be supposed to be of a pure white light-scattering character, reflecting irregularly all the light which falls upon it, and also let P be supposed to reflect, as a convex speculum, all the light which falls upon it. In other words, let their albedoes = 1 in both cases. Then it is obvious that D and P receive equal amounts of light from S, and since they reflect it all off again, in one case by irregular reflection, and in the other case by specular reflection, I was misled at first into supposing that, to an observer situated at any point on the line P - - - S, the point of light seen by reflection on P must be equal in total intensity to the total intensity of the light received from D.

But a little consideration is quite sufficient to throw doubt upon such a conclusion, for the disc D throws all the light back through an angle of 180° to the front of itself, and cannot throw any light behind its own plane, but the light reflected from the polished sphere P is reflected off through an extent of angle of 360°, and this distribution of light through greater angle must necessarily be at the expense of intensity of reflection through the forward 180°, and most likely also at the expense of intensity of the reflection in the direction of P - - - S. However, this can be easily put to the test of more exact calculation.

Let

B = intrinsic brightness of globe S

R = radius of globe S

d = distance representing centres of D or P and S.

Then the apparent intrinsic brightness of D, having an albedo = 1, will be

$$B\frac{R^2}{d^2}.$$

If now L represents the total apparent brightness of S as viewed from a standard distance d, then the apparent total brightness of D as viewed from S (at the same standard distance d) will be equal to

$$L\left(\frac{R}{d}\right)^2\left(\frac{r}{R}\right)^2 = L\left(\frac{r}{d}\right)^2.$$

Turning now to the case of the polished sphere P we have a miniature image of S formed at i halfway between the centre and periphery of P, and this image subtends the same angle at the centre of P as does the real source of illumination S, and also, by a well-known law of optics, the intrinsic brightness of this miniature image is the same as that of the original S. Therefore the apparent total brightness of the image i as viewed from the standard distance d will be equal to

$$L\left(\frac{\frac{1}{2}r}{d}\right)^2 = L\left(\frac{r}{d}\right)^2 \cdot \frac{1}{4}$$

or one-fourth of the value found for the light from the disc.

I have verified this theorem by a few simple experiments, as follows. A thermometer bulb full of mercury, the diameter of the mercury globe being ·485 inch, was compared with a disc of white paper of the same diameter, both being illuminated from the same source and viewed from a point 20 to 30 feet away. In fig. 2 C is a candle flame, S an opaque screen, B the mer-

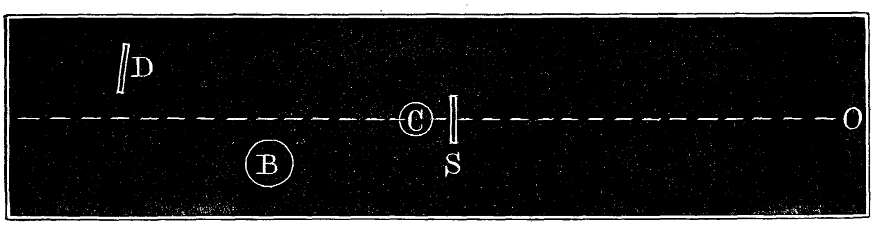

Fig. 2.

cury bulb, D the disc of white paper, and O the position of observation, not less than 20 feet from C or D. The disc was placed to perpendicularly face the light, and both disc and bulb placed as closely as possible to the line of observation C - - - O. The observations were of course made in a darkened room. At the distance C - - - O the disc and bulb presented no very sensible diameter. Then, being short sighted, the light both from the disc and from the specular reflection of the candle flame in the bulb were spread out into a penumbra on my retina, and by a further voluntary shortening of the focus of my eye I could swell out the two lights to be compared, D and B, into two slightly overlapping discs whose apparent diameters so far exceeded the apparent real diameter of D as seen from O as to render the correction due to the greater area of D as small as possible. As the penumbras were generally made to be about ten times the apparent diameter of D, then the areas of the two

apparent lights became 10^2 for D and 9^2 for B, or about 5 to 4. B was placed at from $1\frac{1}{2}$ inches to 3 inches from the candle in different experiments, and then the disc D was adjusted to that distance from the candle found necessary to make the penumbras from the two lights appear of equal intensity as viewed from O.

In three experiments equality was attained when the ratios of the squares of the distances of B from C and D from C respectively were 1 : 3·6, 1 : $4\frac{1}{3}$, and 1 : 3·36.

In another case, with a different mercury bulb in use, the ratio came out about 1 : 4. With 1 to 3, D appeared slightly but distinctly the brightest; with 1 to 5·7 B was distinctly the brightest.

In two other cases a bulb of freshly polished speculum metal of ·37 diameter was tried against a disc of pure white paper of the same diameter, and the results again averaged 1 to 4 or rather under. Zöllner estimated the albedo of white paper to be ·70 (*Photometrische Untersuchungen*, p. 273) and mercury ·65, while Lord Rayleigh has estimated the albedo of mercury to be ·75 at nearly perpendicular incidence (*Phil. Mag.*, October 1892); so I think it will be fair to assume the albedoes of the bulb and of the white paper about equal, or perhaps the reflective power of the mercury slightly lower owing to it being encased in a thin glass bulb. A careful comparison of the two mercury bulbs used and the speculum bulb, as regarded their power of reflecting a sheet of white paper, showed no sensible difference, and speculum metal has been estimated by Sir John Conroy and others to prove an albedo varying between ·60 to ·70. These simple experiments, while not pretending to great accuracy, certainly confirm the theorem that the light reflected back from a polished sphere along the direction of illumination is one-fourth of the light reflected back in the direction of illumination from a disc having the same diameter and perpendicularly illuminated by the same source at the same distance, and supposing the two albedoes are equal. We next have to consider the modification of the amount reflected back when a spherical light-scattering surface is substituted for the flat light-scattering surface of the disc, the diameter of course remaining the same.

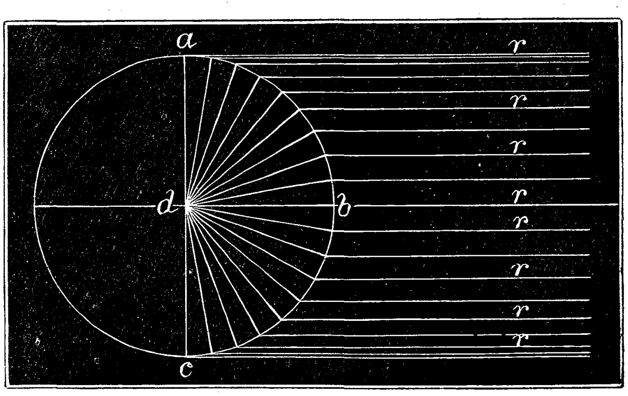

Fig. 3.

Let r, r, r, &c., in fig. 3 be parallel rays from the distant source of illumination falling upon the sphere a, b, c. Instead of a disc a - c receiving all these rays at perpendicular incidence, we must treat the surface of the hemisphere as cut up into a number of equal zones having their edges parallel to the plane or disc a - - c and having the common axis d - - b, which zones get illuminated at different degrees of incidence, and whose apparent widths and areas as viewed from the direction r - - r show large variations.

If the quadrant a - d - - b is divided into n equal parts, and the area of the base (or disc a - c) or $r^2\pi$ is supposed to equal 1 and to reflect light equal to 1 along the direction d - - r, then we have the sum of the following series as representing the total apparent luminosity of the whole hemisphere as viewed from a distant point on d - - r :

$$r^2\pi\left[\left\{1-\sin^2\left(\frac{n-1}{n}\right)90^\circ\right\}\cos\left(\frac{n-\frac{1}{2}}{n}\right)90^\circ\right.$$
$$+\left\{\sin^2\left(\frac{n-1}{n}\right)90^\circ-\sin^2\left(\frac{n-2}{n}\right)90^\circ\right\}\cos\left(\frac{n-1\frac{1}{2}}{n}\right)90^\circ+\&c.,$$

&c., to n terms. And, if $r^2\pi=1$, the total value of the series is $\frac{2}{3}$. This agrees perfectly with Zöllner's two theorems; first as to the apparent illumination of a sphere being equal to that of a cylinder of same radius but of a height equal to $\frac{2}{3}$ of the diameter of the sphere, and having its axis perpendicular to the direction of viewing and illumination; and, second, the apparent illumination of a cylinder with its height *equal* to its diameter, and axis perpendicular to direction of viewing and illumination being equal to the apparent illumination of its circular base when perpendicularly illuminated. Therefore the apparent brightness of the solar reflection off water surfaces in *Mars* in terms of the total apparent brightness of *Mars* will be

$$\frac{3}{2}\cdot\frac{1}{4}\cdot\frac{a}{A}F$$

where

a = the albedo of water

A = the general albedo of *Mars*

and

F = that fraction of the total incident light which is left after two passages through the Martian atmosphere.

Here A and F are both uncertain factors. Zöllner gives ·267 as the general albedo of *Mars*, but Seidel gives only ·09 or so; while Professor Pickering, as the result of careful photometric measurements of the relative brightness of *Mars* and *Saturn* when at or

near conjunction in 1877, came to the conclusion that the albedo of *Mars* was not more than $\frac{1}{4 \cdot 6}$ of that of *Saturn*. If, therefore, we suppose the albedo of *Saturn* to be as high as that of freshly fallen snow, which Zöllner estimated as ·78, then the albedo of *Mars* comes out ·17. I will in the above formula assume ·21 to be the correct albedo for *Mars*.

The albedo of water for all angles of incidence up to about 20° has been proved to be about ·021 (Lord Rayleigh, *Phil. Mag.*, October 1892), and I will assume the fraction of light left after two passages through the perpendicular thickness of the Martian atmosphere to be $\frac{2}{3}$, in view of the strong evidence existing to show that its atmosphere is considerably thinner than our own. Then the above formula gives a value of $\frac{1}{40}$ as the ratio between the intensity of the solar reflection from a water surface in *Mars* and the total brightness of the whole Martian disc. Now Zöllner's photometric measurements of the apparent brightness of *Mars* gave it a relative brightness as compared with *Capella* of 8 to 1 when *Mars* is in mean opposition. But *Capella* is of 0 magnitude, therefore *Mars* in mean opposition is of an apparent brightness equal to twenty times that of a 1st magnitude star. Therefore the solar reflection from water in *Mars* at mean opposition should have an apparent brightness equal to half that of a 1st magnitude star, or $\frac{1}{5}$ of *Capella*. Schiaparelli's calculations, as we have seen, made it out to be equal to $\frac{1}{4}$ of *Capella*, with *Mars* in close opposition.

But a star equal in brightness to half a 1st magnitude star is a brilliant and conspicuous object even in a 2-inch telescope. Here a simple experiment can be made very useful and suggestive. I took an empty thermometer bulb made of thin opal glass measuring 1·4 inches in diameter. This glass is of the ordinary refractive index of about 1·53 or 1·54, and is impregnated, especially towards the inner surface of the bulb, with some white light-scattering substance which gives the whole bulb a resemblance to a globe of milk. On comparing its albedo or light-scattering power with that of pure white paper it seemed rather inferior, so that I should judge the albedo of the bulb to be about ·5. But the polished *surface* of the bulb gave a specular reflection of the source of illumination, and the amount of light reflected in this way would be that usually reflected by a glass surface. Its specular reflection was compared with, and found equal in intensity to, that from the first surface of a crown glass lens whose other surface was backed up by white paper. For an index of 1·54 the amount reflected at approximately perpendicular incidences would be represented by Young's formula

$$\left(\frac{\mu - 1}{\mu + 1}\right)^2 = \left(\frac{\cdot 54}{2 \cdot 54}\right)^2 = \cdot 045.$$

And if any of the white light-scattering particles happened to lie in or upon the actual glass surface they would obviously diminish

the amount of light reflected regularly, so that $4\frac{1}{2}$ per cent. represents the utmost amount of the incident light which is regularly reflected. This white bulb was then placed in a dark room and illuminated by a candle flame placed six inches to a foot or more away and as nearly as possible in the line of sight, the flame being screened in the direction of observation. On receding to some distance a white globe was visible in which the minute image of the candle flame stood out boldly like a star, and this star was too conspicuous to escape naked-eye vision at a distance of 30 feet, where the bulb would subtend about 13 minutes of arc. Here the relation between the albedo of the white surface and the true glass surface giving the specular reflection was $\frac{\cdot 50}{\cdot 045}$, or about 11 to 1, whereas in *Mars* we concluded that the relation between the general albedo of its surface and the albedo of water was $\frac{\cdot 21}{\cdot 021}$, or 10 to 1; or 15 to 1 after allowing for an absorption of $\frac{1}{2}$ for the double passage of the solar rays through the Martian atmosphere. Therefore the two cases are not very materially different, and a magnifying power of about sixty on any good telescope, enlarging *Mars* to an apparent diameter of say 18 minutes, would be quite sufficient to render the solar reflection easily visible. Yet, however difficult it may be to suppose that the water surfaces of *Mars* are never calm enough to give a defined reflection of the Sun, it is nevertheless advisable to consider the effect of moderate waves upon the image. To *Mars* at mean distance the Sun subtends about 21 minutes of arc, and its image reflected off a spherical water surface in *Mars* would appear to lie halfway between the centre and circumference of the planet, and would subtend 21 minutes of arc at *Mars'* centre. Therefore the angular diameter of the solar image in *Mars* as viewed from the Earth at average opposition would be, if the water surface were still, represented by

$$\frac{d_{\theta}}{4}$$

where $d =$ the apparent diameter of *Mars* in seconds and $\theta =$ the circular measure of 21 minutes. This comes out only ·0275 seconds of arc, or so small that it would need a telescope of 15 feet aperture at least to show any appreciable disc.

The absolute size of the solar image in miles would be equal to one quarter of the Martian diameter multiplied by θ or $1050.\theta$; that is, about $6\frac{1}{2}$ miles. Therefore lakes as small as this, if they occurred within the Martian tropical zone, should be capable of throwing us an unimpeded reflection of the Sun. But $6\frac{1}{2}$ miles is far narrower than the finest of the so-called canals of *Mars*. But we have never seen any flash of sunlight from them. It must be remembered too that small sheets of water are not so likely to be troubled with waves as oceans, and even if perturbed, subside again into calmness much sooner.

Let it be supposed, however, that the water surfaces were cut up into waves, whose sides at their greatest slope were inclined at an angle β to the Martian horizon. Then the effect would obviously be to spread out the solar reflection over an angular extent of the Martian surface equal to 2β. How much would it have to be spread out before it would quite escape scrutiny? This cannot be answered except in very general terms. Experiment leads me to think that an inclination of the steepest part of the wave to the horizon of about $14\frac{1}{2}°$ may be possible before the diffused patch of reflected light scattered over 29° of Martian surface would cease to be visible in a powerful telescope giving a large and bright image of *Mars*.

I took the same opal bulb as before, and, instead of illuminating it by a small candle flame, I illuminated it by a square sheet of white paper, reflecting the candle light on to the bulb.

In this way the ratio between the total light scattered by the opalescent substance of the bulb and the total light regularly reflected by the glass surface in the form of a broad patch of light (being a distorted image of the white paper) would not materially be disturbed, but instead of a luminous point, a broader and less intense image was formed. And I found that even when the sheet of paper was large enough to give a reflection in the bulb equal in apparent diameter to one-fourth of the apparent diameter of bulb, the approximately rectangular patch of light showed up in a quite noticeable manner against the duller background. And I do not see any reason for supposing that the effect would be essentially different, if, instead of diffusing the illuminant itself, we diffuse the image of that illuminant by the presence of corrugations on the reflecting surface, having a maximum inclination of sides to the horizon of $14\frac{1}{2}°$, whose sine is $\frac{1}{4}$.

But such waves as these are only what would occur in a fairly rough, if not a very rough sea. Shallow seas do not develop great waves so readily as deep ones, and what waves do arise soon subside again. Some recent telescopic observations of *Mars*, made under the most favourable atmospheric conditions prevailing at the loftily situated observatories in America, notably Mr. Percival Lowell's observatory in Arizona, certainly confirm very strongly the great variability in the outlines and depth of colour of the so-called seas of *Mars*. One cannot read Mr. Percival Lowell's articles on *Mars* in *Astronomy and Astrophysics*, detailing the great changes which he observed to take place as the summer advanced in the southern hemisphere of *Mars* during the late opposition, without being struck by the fact that the darker greenish markings of the disc, from being scarcely discernible and of generally small extension at the spring equinox, develop steadily both in depth of tint and extension to a most marked degree until what we have every reason to regard as the hottest part of the Martian summer is past, when they shrink in size and fade in colour until in the autumn

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they have again relapsed into comparative obscurity. And this takes place in an even more marked degree in the case of the "canals."

If these dusky markings are really seas, then it is very obvious that they must be for the most part extremely shallow, or their extraordinary variations in area cannot be accounted for. And if shallow, then we have little reason for supposing them to be generally much agitated. Besides, even supposing that the atmosphere of *Mars* contains as much gas per square mile of surface as our Earth does, yet its density or pressure would be only two-fifths of that of our atmosphere, while its vertical movements under variations of temperature would have to take place on many times the scale of our Earth in order to produce the same amount of cooling and storm-generating effect. And, although the weight of water would be only two-fifths, yet its inertia and internal friction would be the same as with us, while, for a given wind velocity, the wind *pressure* would be considerably less than with us. Altogether it is extremely probable that the atmospheric disturbances of *Mars* are very mild affairs compared with our own. Mr. E. W. Maunder has very ably discussed the question of the climate of *Mars* in *Knowledge*, vol. xv., and arrived at much the same conclusion. We have therefore good reason for expecting any water surfaces existing on *Mars* to be generally in a calmer state than on our own planet.

Yet it is a very common thing in the case of our own sheets of water to see the Sun's image reflected with no greater unsteadiness than will cause oscillations of its image through five to ten times its own apparent diameter, and this amount of movement would, in the case of the Martian solar reflection, make no perceptible difference to the image as seen in an 18-inch refractor; it would still appear as a star.

Now some of the most well-marked "seas" of *Mars* happen to be well situated for sending us reflections when the Martian South pole is tilted towards us during a favourable opposition, like last year's! From Schiaparelli's *Mare Cimmerium* (longitude 200) eastwards to the E. border of the *Auroræ Sinus* (longitude 60) for 220 degrees of longitude altogether, there is an only slightly interrupted series of so-called seas within a belt lying between 20° and 30° of South latitude, and therefore coming about Martian midday into the plane containing the centres of the Sun, *Mars*, and the Earth. But not a trace of a glint seems to have ever been observed.

The Martian atmosphere cannot be supposed to appreciably interfere with the visibility of the solar reflection, if it exists. I have allowed for an atmospheric absorption of one-third of the light in the above calculations. For, while the planet's atmosphere may absorb more or less light in the sense that glass does, yet we cannot suppose that it also scatters and diffuses light, and thus interferes with the definition of details seen

through it, as our own atmosphere does on a hazy day. For, during the summer, at any rate, of *Mars'* southern hemisphere, the visibility of such extremely fine details as the more difficult "canals" of *Mars* is fairly constant, and depends altogether upon our own atmospheric conditions being favourable.

The dark zone which so closely follows up the retreating edge of the S. polar cap of *Mars* as the summer advances, has usually been considered to be a belt of water resulting from the thawing of the polar snows. But what proof have we that the white polar cap consists of snow of sufficient depth to cause such extensive inundations as would be indicated by the dark-greenish areas lying around the S. pole, supposing they are water surfaces? The rapid melting and shrinkage, and in some cases total disappearance, of the polar snows each summer, is in itself strong evidence of the small depth of the snow deposit (or hoarfrost deposit, or whatever it is), for the efficiency of the Sun's heat for melting snow in *Mars* is barely a half of what it is with us. If we suppose,* then, that the dark-greenish areas which develop and spread in the S. polar regions of *Mars* as the summer progresses are really sheets of water resulting from the thaw, we must obviously assume a most extraordinary degree of flatness in the surfaces inundated, for we could not assume more than two or three feet of depth of water on the above hypothesis, as the greenish areas around the pole far exceed the area of the snow cap, which in itself cannot be more than a few yards thick.

The hypothesis which, I venture to think, best accords with the facts is suggested by the case of our own Earth, where a zone of revivifying vegetation of a dwarf character closely follows up the melting of the snows which have formed a temporary extension of the true polar ice cap during the winter. It is this zone of vegetation, within and about the Arctic circle, which annually becomes the principal breeding ground of a very large number of migratory birds, who find their food either in the herbage and vegetation itself or else in the insects, &c., which live among it. Such a zone existing in *Mars* could scarcely escape observation as a region of a darker and more greenish colour than the rest of the surface. And, seeing that we have negative proof of the fact that the areas having the same colour and appearance, but lying within the planet's tropical regions, are not water surfaces, we may assume as most likely that the greenish colour of all the darker markings of the planet is caused by the presence of vegetation growing in the more low-lying regions where there is sufficient moisture existing in the soil, while the brighter and ruddy-tinted regions are barren desert. The observed seasonal changes in the markings would thus be accounted for.

The Rev. Edmund Ledger, in a recent lecture, discussed the

* Obviously, we could not apply the test of presence or absence of a solar reflection to sheets of water outside the tropical regions of the planet.

absurdity of supposing that the canals could be looked upon as artificial waterways in themselves, for they must be at least 20 miles in diameter to be discerned at all, but he suggested that they might indicate the lines of canals, the visible broadening out being due to cultivated tracts on either side of the main waterway, which should show a darker and greenish tinge. But, if not canals, they might indicate the lines of roads cut straight across the deserts from one centre of population to another. It would be obviously the aim of any inhabitants to make the roads as straight as possible in a nearly waterless planet. Then, if tracts of country were cultivated and made to support vegetation, surely the districts immediately surrounding the centres of population (the dots seen where the canals intersect) would first be attended to, and afterwards substantial strips of desert at each side of the roads would be reclaimed and covered with vegetation. This is one hypothesis which might help to explain the suspiciously artificial-looking and designed appearances of the so-called canals of *Mars*.

The remaining question, of the relative brightness of the solar reflection when *Mars* is in other positions than in opposition, may be shortly dealt with ; for the relative configuration of the orbits of the Earth and *Mars* is such that two lines, one joining the Sun and *Mars* and the other joining the Earth and *Mars*, never enclose a greater angle than about 50°, so that the maximum angle of incidence and reflection would be 25°. But it has been proved that the percentage of light reflected from water at that angle is not very appreciably greater than that reflected perpendicularly. The contrast between the solar reflection and the gibbous disc of *Mars* would thus remain much about the same as when in opposition.

Photographs of the Spectrum of the Ball and Rings of Saturn.

(*Letter to the Secretary.*)

E. W. Maunder, Esq.,
Secretary of the Royal Astronomical Society,
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Dear Sir,—I send you with this a photograph of the spectrum of *Saturn*, which it may interest some of the Fellows of the Society to examine. The fact that the lines in the spectra of the ansæ do not follow the direction of the planetary lines, but are very slightly inclined in the opposite direction, is a spectroscopic proof of the meteoric constitution of the ring, as I have explained at length in the May number of the *Astro-physical Journal*. Although the positive enlargement which I send is less satisfactory than the original negative viewed under a microscope, the feature mentioned above can be seen without difficulty.